But I have been informed that at the whaling settlement of Tawaite, on the east entrance of Tory Channel, from six p.m. up to about eight p.m. on the evening of the 9th (the night preceding the eruption), loud booming reports were heard as through the earth. As these reports were previous to any symptom of the loud disturbances at Tarawera, this suggests that they may have resulted from a slight movement along the great fault-lines that traverse the North and South Islands in a north-easterly direction; and, in this case, the immediate cause of the Tarawera outburst may be found in a local fracture resulting from such movement.

"VIII. *Premonitory Symptoms.*—The only premonitory symptoms of the coming outburst which have been described were an oscillation in the level of Tarawera and Rotorua Lakes, and the occurrence of earthquakes for some months past in that district, where, as a rule, earthquakes are rarely felt. But neither of these are very characteristic incidents, nor would it be safe on future occasions to base any expectation of an eruption on such phenomena alone. The increased activity of the geysers and hot springs during the past season has also been advanced as having been a symptom of an approaching outbreak; but those who were most familiar with the district will agree that their variation was no greater than is usual under the influence of rapid changes of wind and atmospheric pressure. The reports of sympathetic outbreaks in other places along the line of volcanic energy from White Island to Ruapehu appear to be quite unfounded. The outburst has shown conclusively that the springs at Rotorua and Rotomahana are quite independent of each other, and of those at other places, thus confirming the observations made by Von Hochstetter long ago, that all the various points at which thermal springs occur are situated round the margins of lakes formed by subsidence of circular areas, and are not connected by an underground system of gravitational drainage.

"IX. *Conclusion.*—From the foregoing sketch of the character of the eruption I think there can be little question that it is a purely hydro-thermal phenomenon, but on a gigantic scale; that it is quite local and not of deep-seated origin, and that all danger is past for the present, so far as one can venture to form an opinion on such a subject. The extra activity of the *puias* which has been observed is no doubt owing to the heavy rains that, on the 9th, set in after the longest period of drought which has been experienced in that district for many years, and probably the frequent earthquakes which have of late agitated the ground have contributed to this activity by stirring up the sources of the water-supply, and facilitating the access of drainage-waters to the sources of the heat. But beyond what may be accounted for in this manner I believe there is no increased disturbance at Rotorua, Wairakei, Taupo, and other places. The quiescent condition of Tongariro and Ngaurahoe was plainly shown by the manner in which we observed it to be enveloped in snow. As a rule, on the scoria cone of Ngaurahoe, snow rarely lies, excepting in a few of the gullies, but melts almost as rapidly as it falls. On the morning of the 17th, however, the cone of Ngaurahoe was covered with a great mantle of snow; while the *puias* on Tongariro showed less than their usual amount of steam escaping. The only fresh activity which may be reasonably expected is that which I anticipate when sufficient rain has fallen to cause the overflow of Okaro Lake into the south end of the great fissure, as its former drainage outlet to the Rotomahana Lake appears to me to be completely filled up. If this should occur, and a fresh explosion takes place in consequence, it will be comparatively moderate in its effects, as, unlike Rotomahana, the soft, incoherent pumice deposits between the fissure and Okaro Lake are not sealed down by an enormous weight of siliceous sinter.

"For some time to come great variations must be expected in the activity of the newly-formed *puias* according to the manner in which changes occur in the atmospheric pressure; but, unless it can be shown that any local change in the barometer is experienced which is not shared by the surrounding district, the barometer affords no indication as to whether an eruption is or is not imminent. One of the most unfortunate results of the eruption, in addition to the disastrous loss of life and the destruction of the country, is the disturbance of the sense of security which has grown up amongst those residing at the Hot Springs; and I believe that many persons are so thoroughly shaken by the horrors experienced on the morning of the 10th that they will not recover their equanimity until they have been for some time resident away from the sounds, smells, and shocks that characterise the district. "JAMES HECTOR"

## *IN QUEST OF THE ORIGIN OF AN EPIDEMIC*

IN our issue of the 8th ult. (vol. xxxiv. p. 213) we dwelt on certain general aspects of the reports lately laid before the President of the Local Government Board by the Medical Officer of the Department on milk-scarlatina, but these documents deserve more detailed consideration, for they show us our modern organisation for combating death and disease, by prevention, at its best. They show us, too, the men to whom the task of guarding public health is primarily committed at their best—patient, watchful, wary, tenacious of the thread of their investigation, eliminating this or that doubtful element, until finally they have tracked their quarry to its lair. In reading Mr. Power's report, we have been constantly reminded of that famous description of the contest between the man and the gun in Hugo's "Toilers of the Sea." Here the fight was man against disease, and the former has succeeded in his task. We shall endeavour in this article to show how Mr. Power, of the Local Government Board, succeeded in tracing, step by step, an epidemic of scarlatina to its source.

On December 18, 1885, Mr. Winter Blyth, the Medical Officer of Health of St. Marylebone, reported to the Board an extensive outbreak of scarlatina in his district. This he believed to be associated with the distribution of milk from a certain retailer in South Marylebone, who obtained his supplies from two farms, but the occurrence of the scarlatina appeared to be coincident with the milk-distribution from a certain farm at Hendon. Mr. Blyth had himself visited this farm, and, with the assistance of Dr. Cameron, the Hendon Medical Officer of Health, had carefully examined it, but was quite unable to discover in its sanitary circumstances or in the health of those employed about it any sort of clue to the cause of the infection of the milk. Accordingly he went with his story to the Local Government Board. It will be seen that Mr. Blyth had done his work exceedingly well: in one of the most crowded districts of London he had succeeded in tracing the scarlatina to a farm at Hendon; that is, he had made out a strong *primâ facie* reason for suspecting this farm; he had put a clue into Mr. Power's hands which he had not been able to follow any further himself. The first question for Mr. Power to answer was whether the Hendon farm was at fault or not. When this was answered it would be time enough to pursue the inquiry more minutely: it would be loss of time to try to dig out the fox unless it was first ascertained that he was in that particular earth. With this object, then, Mr. Power traced the milk from the Hendon farm to other milk-retailers in St. John's Wood, St. Pancras, Hampstead, and Hendon itself. From each of these, except St. John's Wood, the same story came. Until the end of November or beginning of December the district had for some months been exceptionally free from scarlatina, but about

this date the disease had suddenly and notably increased, a large proportion of the recorded cases having occurred amongst the customers of milk-retailers dealing in the particular Hendon milk. These facts strengthened the case against the Hendon farm, but did not by any means establish it, inasmuch as the retailers in question obtained their supplies from other farms as well, and although in two cases these were situated in widely different counties, yet the case against Hendon was still in the condition of not proven, more especially as the St. John's Wood customers of that farm were certainly wholly free from scarlatina. Simultaneously with this investigation, another was being pursued at the incriminated dairy itself. But nothing was revealed here to show how the disease could be propagated from it as a centre. There was no scarlatina, nor any illness at all like scarlatina, amongst the persons employed about the farm, or their families and neighbours, at any such time or in any such way as to influence the farm or its produce. This, then, was the state of affairs on December 23, or less than a week after Mr. Blyth's report: there was a strong presumption against the Hendon farm, but outside human agencies had to be set aside as not having been operative. A thorough inspection of the farm itself was at once undertaken. Now it happened that the farmer in question, as well as one of the dealers who purchased from him, was particularly careful in all sanitary matters respecting his dairy. Every precaution had been taken by both to secure the farm and milk against any known sanitary fault or misadventure, and thus the inquiry advanced another stage. If the Hendon farm had caused the scarlatina, it did not do so in any commonly accepted way, such as through unwholesome conditions of water or drainage, or careless handling of milk or milk-utensils, by persons carrying scarlatina infection. This threw Mr. Power back on the theory of something in the cows themselves which caused the scarlatina to be distributed with their milk, and this formed his working hypothesis thenceforth. To discover this "something," and to understand its nature, it was necessary to ascertain in detail every parallel between the doings at the dairy farm and the observed scarlatina.

Here, then, we enter on the second and by far the most difficult stage of the investigation. The various districts supplied from Hendon were taken one by one; the quantities of milk obtained from Hendon by the dealers there, and by the same dealers from other sources, were ascertained; the dates of the notable incidence of the disease among the customers, and the degree of incidence at one period and another, were carefully observed, and compared, with the following results:—(1) The disease commenced at one and the same time in the four districts supplied from Hendon, viz. South Marylebone, Hampstead, St. Pancras, and Hendon. (2) In South Marylebone the disease increased day by day with increasing force up to the date of the inquiry. (3) In Hampstead and St. Pancras there was a cessation of ten days after the first attack, and then a larger number of persons were taken ill, the attacks continuing up to the date of the inquiry. (4) In St. John's Wood there was no scarlatina whatever down to the date of the inquiry, although the dealer there got five-sixths of his milk from Hendon. Were there any conditions in the farm operations parallel to these special phenomena? And first, was there any new condition pertaining to the cows coincident with the milk producing scarlatina at the end of November in four districts, continuously in South Marylebone, and after a break in the other three, while this condition was absent in the case of the cows supplying the St. John's Wood dealer? A tedious inquiry into such circumstances as the food, calving, health, arrival and departure of cows proved barren of result; nothing could be heard of for some time that was new or changed. But at last it appeared that on November 15 three newly-calved cows, purchased in Derbyshire, had come on the farm, and four from Oxfordshire on December 4. The practice of the farm was to isolate or quarantine new arrivals for examination for a week or ten days, and then to admit them into the stalls with the others. The cows on the farm at this period numbered 90 or 100, distributed in unequal numbers in three sheds, called the large, middle, and small sheds. The supply of the milk from the large shed went to South Marylebone only; that from the middle shed partly to South Marylebone, partly to Hampstead and St. Pancras; and that of the small shed to the two latter places and to St. John's Wood. So far we have this coincidence between the doings at the farm and the incidence of the disease—that the latter broke out after the time that the milk of the Derbyshire cows was added to the general stock, in three districts supplied from the farm; and that St. John's Wood, which did not receive any milk from the new arrivals, was free from scarlatina.

We have now reached what may be called the third stage of the case. In the first, what Mr. Power calls a "notable," and what lawyers perhaps would call a "violent," presumption had been made out against the Hendon dairy; in the second, a weaker presumption had been established against the Derbyshire cows which had been added on November 15, and whose milk began to be distributed to the three affected districts, and not to St. John's Wood, a few days later. But then, the facts of a continuous and increasing attack in South Marylebone, and the intermission of about ten days in St. Pancras and Hampstead, had to be accounted for, if the case was to be made out conclusively against the incriminated dairy. To deal with these, Mr. Power reversed the process hitherto pursued, which was that of pure induction from observed facts. He now employed the *a priori* process, and argued thus:—Taking the fact of uninterrupted progress of the disease in South Marylebone, and of the lull of ten days in the other two, if the dairy at Hendon be the cause of the outbreak, and if, as is most probable, the different results produced by the milk from the same cows was due to a difference in the relation of the cows themselves within the business of the farm, then we should find at the latter—(1) a change in the manner of distributing the milk of the Derbyshire cows, and this probably consisting in placing them, or one of them, in the "large shed," from which South Marylebone was supplied; (2) about the second week in December (the date of the recrudescence of the disease in St. Pancras and Hampstead), some of the Derbyshire or of the Oxfordshire cows, or some other cows which had been in close relation with them, were probably transferred to the "middle shed," from which these two districts were, it will be remembered, supplied; (3) as St. John's Wood, which was supplied from the "small shed," was free from scarlatina, it should be found that none of the new cows, or any other cow in close relation with them, had been placed there. Now, were any arrangements at the farm found corresponding with any or all of these *a priori* conclusions or probabilities? What was found on investigation was this: (1) The Derbyshire cows had been transferred towards the end of November into the "large shed" (the source of the South Marylebone supply), and remained here at the date of the inquiry; (2) the four Oxfordshire cows were transferred about December 11, two into the "large shed," and two into the "middle shed" (St. Pancras and Hampstead supply); (3) at no time had either the Derbyshire or Oxfordshire cows been transferred to the "small shed" (St. John's Wood). Here, then, both by positive and negative evidence, the presence of scarlatina in certain London districts was associated, first, with a particular dairy, and secondly, by a series of parallel events, with certain cows within that dairy. Mr. Power, having reached this point, felt justified in assuming, until anything to the contrary should appear, the presence

of something in these cows competent to produce scarlatina in persons consuming their milk, and the inquiry was narrowed to determining what this was. All comparison with former experiences was for the present left out of consideration, the investigation proceeding strictly on the circumstantial evidence obtained and obtainable. A consideration of all that had gone before, and the absence of any alternative, led to the provisional adoption at this point of a theory of disease in the cows, and the probability was that this was an infectious disease, communicable from cow to cow, a disease, moreover, the existence of which was compatible with the animal affected feeding well, and milking abundantly.

The discovery of vesicles and ulcers on the teats and udders of cows in the large shed soon followed; the first to show the disease was one of the Derbyshire cows, the second one from Oxfordshire. After this the matter passed into Dr. Klein's hands; but with his report we have nothing to do here. A painful incident soon gave Mr. Power ample corroboration of the result which he had reached. The Marylebone dealer returned on the farmer's hands, on December 15, all his milk from the larger shed, and this was destroyed by pouring it into a pit dug on his land. The news of the destruction of milk spread among some of the poor people of Hendon, and some of them succeeded by the favour of friends amongst the cowmen in obtaining some of it on December 16. By the 20th scarlatina made its appearance amongst half-a-dozen of the families thus supplied. Conversely in South Marylebone about Christmas, when these Hendon families were falling ill, the disease ceased almost suddenly, and there were no fresh attacks, except such as were referable to infection from previous sufferers.

A thorough examination of all the cows showed that the disease had spread to every one of the three sheds, and the farmer was accordingly advised to seek out every cow then or afterwards affected with sore teats or udder, or any other ailment, to isolate her and keep all her milk out of the business, and prevent cowmen employed about the sound cows from attending the infected ones. These precautions were taken from January 1, and were barely in time to prevent an alarming increase of scarlatina in all the districts served from Hendon, including St. John's Wood, where the appearance of scarlatina corresponded to a nicety with the appearance of the cow-disease in the animals in the small shed. The milk from the Hendon farm was ultimately given up by all the dealers concerned, with the result that scarlatina has disappeared from amongst the customers of the dealers here referred to in Marylebone, St. Pancras, Hampstead, and St. John's Wood. The work of demonstrating the nature of the cow-disease, and its connection with human scarlatina was not Mr. Power's, and from him the matter passed on to Dr. Klein. The former had succeeded in gathering up and connecting the scattered links of a chain of presumptive evidence against certain cows so strong as to be unassailable; and he had done this by the exercise of patience, sagacity, and acuteness which would have done credit to a great criminal lawyer weaving the web of circumstantial evidence around an unusually cunning forger or murderer.

## *THE ORIGIN OF VARIETIES*

THE publication in the three last numbers of NATURE, by Mr. Romanes, of very important papers,[1] induces me to send the following lines as a contribution to the discussion upon them that is sure to ensue. He ascribes the origin of varieties to peculiarities in the reproductive system of certain individuals, which render them more or less sterile to other members of the common stock, while they remain fertile among themselves.

[1] I write from abroad, and have not yet seen the original memoir published by the Linnean Society.

I also have a theory which, while it differs much from that of Mr. Romanes, runs on curiously parallel lines to it, and was prompted by the same keen sense of an inadequacy in the theory of Natural Selection to account for the origin of varieties. I should not have published my views until they had been far more matured than they are had not the present occasion arisen.

It has long seemed to me that the primary characteristic of a variety resides in the fact that the individuals who compose it do not, as a rule, *care to mate* with those who are outside their pale, but form through their own sexual inclinations a caste by themselves. Consequently that each incipient variety is probably rounded off from among the parent stock by means of *peculiarities of sexual instinct*, which prompt what anthropologists call endogamy (or marriage within the tribe or caste), and which check exogamy (or marriage outside of it). If a variety should arise in the way supposed by Mr. Romanes, merely because its members were more or less infertile with others sprung from the same stock, we should find numerous cases in which members of the variety consorted with outsiders. These unions might be sterile, but they would occur all the same, supposing of course the period of mating to have remained unchanged. Again, we should find many hybrids in the wild state, between varieties that were capable of producing them when mated artificially. But we hardly ever observe pairings between animals of different varieties when living at large in the same or contiguous districts, and we hardly ever meet with hybrids that testify to the existence of unobserved pairings. Therefore it seems to me that the hypothesis of Mr. Romanes would in these cases fail, while that which I have submitted would stand.

The same line of argument applies to plants, if we substitute the selective appetites of the insects which carry the pollen, for the selective sexual instincts of animals. Both of these, it will be remembered, are mainly associated with the senses of smell and sight. If insects visited promiscuously the flowers of a variety and those of the parent stock, then—supposing the organs of reproduction and the period of flowering to be alike in both, and that hybrids between them could be produced by artificial cross-fertilisation—we should expect to find hybrids in abundance whenever members of the variety and those of the original stock occupied the same or closely contiguous districts. It is hard to account for our not doing so, except on the supposition that insects feel a repugnance to visiting the plants interchangeably.

No theme is more trite than that of the sexual instinct. It forms the main topic of each of the many hundred (I believe about 800) novels annually published in England alone, and of most of the still more numerous poems, yet one of its main peculiarities has never, so far as I know, been clearly set forth. It is the relation that exists between different degrees of unlikeness and different degrees of sexual attractiveness. A male is little attracted by a female who closely resembles him. The attraction is rapidly increased as the difference in any given respect between the male and female increases, but only up to a certain point. When this is passed, the attraction again wanes, until the zero of indifference is reached. When the diversity is still greater, the attractiveness becomes negative and passes into repugnance, such as most fair-complexioned men appear to feel towards negresses, and *vice versâ*. I have endeavoured to measure the amount of difference that gives rise to the maximum of attractiveness between men and women, both as regards eye-colour and stature, chiefly using the data contained in my collection of "Family Records," and have succeeded in doing so roughly and provisionally. To determine it thoroughly, and to lay down a curve of attractiveness in which the abscissæ shall be proportional to the amounts of difference, and the ordinates to the strength of attraction, would require fresh and special data that have